

Comment on the proposed conservation of the specific name of *Callidea lateralis* Guérin-Méneville, 1838 (currently *Lamprocornis lateralis*; Insecta, Heteroptera) (Case 3523; see BZN 67: 213–217, 314)

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We studied carefully the detailed argumentation of Tsai & Rédei (2010; *Zootaxa*, 2572: 25–47) concerning *Lamprocoris obtusus* (Westwood, 1837), a senior objective synonym of *Lamprocoris lateralis* (Guérin-Méneville, 1838). There is no doubt about the following facts emphasised by Rédei & Tsai (BZN 67: 213–217):

i) The senior name *L. obtusus* was greatly overlooked by subsequent authors and has never been positively treated as different from *L. lateralis*;

ii) There is extensive and substantial literature on this biological species under the junior name *L. lateralis*;

iii) This species is of economic importance, and is rather common in several parts of Southeast Asia, so it appears also in the agricultural literature and popular books on insects;

iv) Changing the name of this species simply because of adherence to the Principle of Priority is undesirable and would threaten stability of SCUTELLERIDAE nomenclature.

For these reasons, we strongly support the solution suggested by Rédei & Tsai (BZN 67: 213), i.e. to use the Commission's plenary power to suppress the specific name *Callidea obtusa* Westwood, 1837 for the purposes of the Principle of Priority but not for those of the Principle of Homonymy, as is summarised in paragraph 9 of Case 3523.

Comment on the proposed conservation of usage of *Allosaurus* Marsh, 1877 (Dinosauria, Theropoda) by designation of a neotype for its type species *Allosaurus fragilis* Marsh, 1877

(Case 3506; see BZN 67: 53–56; 178, 255–256, 332)

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The taxonomy of the species referred to *Allosaurus* has been a contentious issue, as summarised by Paul (1988, 2010) and Chure (2000). Bakker (2000) and Paul (2010) claimed that *Allosaurus fragilis* (based on USNM 4734) is distinct from other specimens (DINO 2560, AMNH 666, etc.) by the proportions of its skull. However, Chure (2000) demonstrated that the supposed shortness of the skull of USNM 4734 was based on an erroneous reconstruction of the skull by Gilmore (1920).

Additional references

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Comment on the proposed conservation of usage of *Testudo gigantea* Schweigger, 1812 (currently *Geochelone (Aldabrachelys) gigantea*; Reptilia, Testudines)

(Case 3463; see BZN **66**: 34–50, 80–87, 169–186, 274–290, 352–357; **67**: 71–90, 170–178, 246–254, 319–331)

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Since Case 3463 was submitted to ICZN comments on the name of the Aldabra tortoise have been many and varied. The most recent paper on the issue is by Frazier & Matyot (2010), who extensively comment on the identity of the lectotype of *Testudo dussumieri* Gray, 1831 (RMNH 3231). As former curator of the herpetological collection of the Natural History museum in Leiden, the Netherlands (RMNH), I was rather amazed to see this article, knowing that neither one of the authors ever has seen the specimen discussed, neither during my tenure in the RMNH (1966–2004) nor between 2004 and the publication of their mentioned article. It therefore seems necessary to add some more comments to the already (too) extensive literature of this case.

Identification

Frazier & Matyot (2010) present arguments against the credibility of the data that accompany the lectotype of *T. dussumieri*. They make statements that contradict all we know about the specimen and its history, and even reach the conclusion that ‘it is possible that the last survivors of an endemic species of tortoise were on Mahé at the time of Dussumier’s visits to Seychelles possibly starting as early as 1823, and therefore that his specimen that is now in Leiden [RMNH 3231] is not an Aldabra tortoise but rather a Seychelles tortoise’ (Frazier & Matyot, 2010, p. 41). Note that this conclusion was based on assumptions only and that the authors never studied the specimen. Frazier & Matyot (2010) did not use the photographs of the specimen (RMNH 3231) available in the literature (Gerlach, 2004a; Bour, 2006; Grünwald, 2009) to provide evidence for their remarkable statement about the identity of RMNH 3231, although they were well aware of the existence of these photos (Frazier, 2006b; Frazier & Matyot, 2010). Up until now, RMNH 3231 has been studied probably by six professional herpetologists only: Hermann Schlegel, J.E. Gray, A.A.W. Hubrecht, Roger Bour, Peter C.H. Pritchard, and myself, and more recently by F. Grünwald (2009; BZN **67**: 177), a Dutch tortoise hobbyist. Only the last four of these persons are alive and have participated in the debate on Case 3463. These seven persons independently reached the conclusion that RMNH 3231, based on external morphological characters, was an Aldabra tortoise, *Dipsochelys dussumieri*. Austin et al. (2003) used mtDNA of old type specimens of non-Madagascan *Aldabrachelys* (including the lectotype of *Testudo dussumieri*) to determine their

identity. A piece of alcohol-preserved tissue from the groin was taken by me from RMNH 3231 in 2000 and provided to E.N. Arnold for analysis. This tissue provided a 336 base pair (bp) partial sequence (Austin et al., 2003). There was minimal variation among the sequences studied, although RMNH 3231 (haplotype B) diverged by two nucleotide substitutions (a negligible 0.46% divergence) from the most common haplotype A. Austin et al. (2003) concluded ‘The individuals within the studied sample that differ from the common haplotype by 1–4 bp (0.23–0.9% divergence), including the type of *A. dussumieri*, would also be referable to the same single species, for even closely related tortoise species that are widely accepted show much greater differentiation (see above)’, and ‘On the basis of its uniformity compared with other tortoises, the mtDNA of non-Madagascan *Aldabrachelys* studied here suggests that only a single species may be involved’. Thus an independent method reached the same conclusion as the seven persons mentioned before. This conclusion of Austin et al. (2003) was wholeheartedly subscribed to by Frazier (2006b, 370) in his book review of Gerlach (2004). However, Frazier & Matyot (2010) now cite and interpret the Austin et al. (2003) data differently from Frazier (2006b) in order to ‘support’ their aberrant opinion on the identity of RMNH 3231 (see below).

The statement by Frazier & Matyot (2010, p. 42) ‘. . . considering the very limited information that has been published about RMNH 3231, it has simply been *assumed* that the specimen is an Aldabra tortoise . . .’ is an assumption on the part of these authors that is only based on their prejudiced supposition that RMNH 3231 cannot be an Aldabra tortoise. But without studying the specimen themselves they cannot provide any hard evidence against the independent identifications of RMNH 3231 made so far by competent herpetologists.

It may be noted here that Matyot (BZN 66: 352) was mixing up two specimens when he said that the specimen described by Duméril & Bibron (1835) was the same one as that given to Leiden. First, the Leiden specimen (RMNH 3231) was already in Leiden when Duméril & Bibron (1835) published that description (Gray, 1831b). Secondly, the fact that the specimen described by Duméril & Bibron (1835) is still in Paris under number NMNH 1942 (Frazier & Matyot, 2010), invalidates Matyot’s (BZN 66: 352) observations on this subject.

Name and locality

Hoogmoed et al. (2010) published an account of the type specimens of turtles, tortoises and crocodiles in the Leiden Museum. They provided data on the origin and the locality of RMNH 3231, which are contested by Frazier & Matyot (2010).

Gray (1831a, p. 3) mentioned *Test. Dussumieri*, Schegel [sic]. Hoogmoed et al. (2010) have pointed out that although Gray (1831 a, b) attributed the name *Testudo Dussumieri* to Schlegel, this was not correct. The specimen was received from Paris with that name ‘attached’ to it. This was repeated by Frazier & Matyot (2010), who gave an extensive overview of the early history of this name. Gray (1831b) visited European museums somewhere before 1831, because the preface to his *Synopsis Reptilium* is dated January 1831. In his preface Gray (1831b) explains the rules under which he was allowed to see material: ‘In each of these museums all the specimens were intrusted to me, to describe, draw, or examine them, as might best suit my purpose, without any restraint, except that, at Leyden, Herr Temminck requested I

would indicate in what Museum I had seen it, and the name under which it was there described, a rule which I hope I have most faithfully kept.'

A young specimen of *Testudo dussumieri* was present in Leiden during Gray's visit there, as we can see from the text on p. 9 (Gray, 1831b), where in the synonymy of *Testudo indica* he gives a short description of *Testudo dussumieri*: 'Junior. Testa nigra margine laterali angulato, areolis magnis. Test. Dussumieri, Schlegel MSS. (v. Mus. Leyd.) – Pet. Gaz. t. 76, f. 4.' and also mentions the distribution (and collectors) of the species as 'Habitat in India Orientali, Gefi. Hardwicke, Insula Mauritiana, Insula Aldebra, M. Dussumiere, Galapagos, D. Harlan, Seychelles, (v. v. Hort. Zool., et t. Mus. Brit., Col. Chir., Par., et D. Bell.)'. Gray (1831b) made his reference to Schlegel ['MSS (v. Mus. Leyde)'] at the request of Temminck (see above). MSS is not further explained, but probably stands for manuscript or manuscripts, but we can not be certain of that, it may also have meant a name on a label, on a bottle, on a shelf, or even an oral communication (most likely by H. Schlegel). The 'v' in front of Mus. Leyde undoubtedly stands for 'vide' (= seen [by Gray]). Thus, on one page, we have all essential information (apart from the fact that it belonged to the Leiden collection) about *Testudo dussumieri* together: name, collector and locality. However, unfortunately Gray (1831b) presents his data in such a way that the three can not unambiguously be connected, although circumstantial evidence is strong. It seems important here to highlight another part of Gray's (1831b) preface in which he states: '..... the Royal Museum of Leyden and the Museum of the Senckenbergers Society of Francfort having been formed within these few years, the greater part of the specimens are quite fresh and in the most perfect condition, and their history is generally known and accurately marked upon them.' From this text it is clear that Gray (1831b) had full confidence in the data that accompanied the specimens he saw in Leiden.

Fortunately, concerning *Testudo dussumieri* there is a solid, printed statement that ties specimen, collector and locality together. Hoogmoed et al. (2010) mentioned that Temminck & Schlegel (1834) made a clear statement about the provenance of RMNH 3231: it was received from the Paris museum under the name *Test. Dussumieri* and was brought from the island of Aldebra by Dussumier. This statement in French is cited in full and translated by Bour (2006) and copied again by Frazier & Matyot (2010, p. 33). However, after having copied the clear statement, which does not leave any room for speculation, Frazier & Matyot (2010) start questioning its validity on the basis of confused arguments. Hubrecht (1881) also discussed RMNH 3231 and stated that 'The locality from whence the specimen was brought is sharply fixed. Dussumier himself on his travels in the tropics collected it in the island of Aldabra (N.W. of Madagascar) ...'. This citation in Frazier & Matyot (2010, p. 36) again is followed by the comment that Hubrecht did not explain how he determined the locality of his specimen was 'sharply fixed'.

The answer to all queries of Frazier & Matyot (2010) is very simple and unambiguous: Temminck & Schlegel (1834) made the published, printed statement about name, collector, locality and specimen on the basis of documentation (in whichever form) they had received from Paris with the specimen concerned. Hubrecht (1881) did the same, basing himself on the register and data on the label fixed to the bottle in which RMNH 3231 was (and still is) kept. In the RMNH it always has been good practice to trust the data provided with material, until the

contrary is proven. In this case there was no reason for any doubt, and Gray (1831b) was of the same opinion. Apparently Frazier & Matyot (2010) are unable, or rather unwilling, to accept obvious facts and lose themselves in a maze of suppositions and speculations about a locality that never has been doubted.

Frazier & Matyot (2010) cited Austin et al. (2003) to discredit the locality from whence RMNH 3231 came. They erroneously attributed all statements about *T. dussumieri* in this paper to Bour, who was one of the three co-authors and should not be singled out as being responsible for those data; statements in the paper are the shared responsibility of all three co-authors. Frazier & Matyot (2010) did not cite the reference correctly. They cite Austin et al. (2003) incompletely, and checking that paper carefully gives a completely different picture from the one Frazier & Matyot (2010) try to give. Frazier & Matyot (2010, p. 40) suggest that Austin et al. (2003) doubted the type locality of *T. dussumieri*, because in Table 2 the type locality is indicated with a question-mark. However, in Table 1 and on p. 1419 Austin et al. (2003) list the locality correctly as resp. 'Insula Aldebra' and 'Aldabra'. The question-mark in Table 2 under locality for *T. dussumieri* probably was a mistaken repeat of the one on the line above concerning *T. daudini*. This most likely was due to carelessness in reading the proofs of this article which has several typos that could and should easily have been avoided, e.g. in Table 1 RMNH 3231 is referred to as RMNH 32311, in Figure 2 RMNH 3231 is listed as NMNH 3231, the legend of Table 2 refers to "enBank" instead of GenBank, etc. Thus, there is no reason to accept Frazier & Matyot's (2010) reasoning about Austin et al. (2003) supposedly doubting RMNH 3231's locality.

Frazier & Matyot (2010, p. 38) incorrectly conclude that Hoogmoed et al. (2010) contradict themselves when talking about the type locality of RMNH 3231. What Hoogmoed et al. (2010) in effect were saying, is somewhat different from what Frazier & Matyot (2010) suggest. Hoogmoed et al. (2010) stated clearly that the type locality of RMNH 3231 is Aldabra and nowhere do they doubt this; they only cite two papers that say that even had Dussumier not visited Aldabra he could easily have obtained material from there (Bour et al., 2010; Cheke, BZN 67: 79). Hoogmoed et al. (2010) do not make any statements about whether Dussumier picked the specimen up himself on Aldabra or not, they just show that there is no reason to doubt the locality, because that has been associated with RMNH 3231 from the beginning. And stating (Frazier & Matyot, 2010) that Hoogmoed et al. (2010) had '... accepted Matyot's conclusion that Dussumier did not collect on Aldabra' is stretching the truth a bit too far, to put it mildly. The statements by Frazier & Matyot (2010) on p. 40 '... but it does not remove the uncertainty about the origin of the specimen [RMNH 3231]', on p. 41 that 'If – as all evidence [which evidence do they mean?] indicates – the place of origin of RMNH 3231 is Mahé, or even some other island in the granitic Seychelles, and not Aldabra Atoll ...' and on p. 42 (referring to MNHN 1942 and RMNH 3231) '... when in fact the locality data for both of these specimens are known to be uncertain ...' all can be considered wishful thinking, rather than the result of accumulated scientific evidence. As shown here, none of the arguments of Frazier & Matyot (2010) that RMNH 3231 is not from Aldabra, hold up against the known facts, and the origin of RMNH 3231 undoubtedly remains Aldabra as was accepted from the beginning (Gray, 1831b; Temminck & Schlegel, 1834; Hubrecht, 1881; Gerlach, 2004; Grünwald, 2009; Hoogmoed (BZN 66: 354); Hoogmoed et al., 2010).

Labels

As to the labels and other paper concerning RMNH 3231 there have been some unfortunate statements and mistakes in transcribing handwritten texts. Grünewald (2009, p. 139, upper figure) showed an old label on the outside of the jar in which RMNH 3231 is kept and gave as a legend ‘Het oorspronkelijke label van RMNH 3231, geschreven door John Edward Gray zelf’ [The original label of RMNH 3231, written by John Edward Gray himself]. This statement led Frazier & Matyot (2010) to several wrong conclusions, even after Grünewald explained to them that his text should have included ‘possibly’. There is no reason at all for such a statement, because the RMNH never let (foreign) visitors write labels that were attached to bottles etc. The collection of the RMNH was established in 1820. About the early history of its management we know little and it even is not quite certain when the present numbering system for reptiles and amphibians jointly was started, although there are some clues to that. During my tenure at RMNH I did some investigation into the matter that resulted in a notice I made in the register that was published by Böhme & Koch (2010, p. 62) in translation: ‘numbers up to ca. RMNH 3760 are classified systematically, higher numbers irregular. Up to that [number] it concerns animals received up to ca. 1866. From RMNH 3760 [on] irregular with older specimens (1837) and newer (1872, 1877) [intermixed]. I think that from the end of the 1860’s, beginning 1870’s (RMNH 3881 and further) it seems that specimens were classified on receiving date’. Holthuis (1995) mentioned that A.A.W. Hubrecht, who became curator of Vertebrates, especially fishes, on June 1, 1875 (and left the RMNH in 1882) catalogued the alcohol-collection of fishes and that of reptiles and amphibians (3759 lots). It is not clear from where Holthuis (1995) obtained these data, but the number of lots agrees with the last catalogue number of the systematically arranged reptile and amphibian alcohol material mentioned by me in the remark in the RMNH register above. However, the dates of Hubrecht’s stay at the RMNH do not seem to agree with the arrangement of material in the register. Material received up to 1866 is all arranged systematically, between RMNH 3760 and RMNH 3880 there is a mix of older and more recent specimens, and from RMNH 3881 on material is listed according to arrival date. It therefore seems likely that the registering was done by William Marshall, who was assistant for Vertebrates at the RMNH between 1868 and 1872, and whose active period at RMNH better coincides with the arrangement of material in the register than that of Hubrecht. Another possibility of course remains that Hubrecht indeed started the cataloguing (as Holthuis, 1995 stated), in about 1875–1877, but that would mean that in the period between 1866 and 1877 new material was not inserted in the collection at its proper systematic place and this seems unlikely. I therefore assume Marshall was the one that started the present day catalogue and numbering system of the collection of reptiles and amphibians of the RMNH. *Testudo dussumieri* is registered in the middle of a bunch of other species of *Testudo* in the first part of the RMNH register, where specimens are arranged systematically. This shows the specimen was present by about 1870 when the numbering system started. The text in the handwritten register is clear and unambiguous and reads as follows: ‘*Testudo elephantina* juv. *Test. indica* Ile Aldabra (pres de Madagascar)’, and written above ‘(pres de Madagascar)’ is the name Dussumier. There are no alterations or deletions in the text, only *elephantina* is underlined, the meaning of which is not clear. This same information can be found

written on the old label on the outside of the jar in which RMNH 3231 is kept, in the same hand, with the exception of the name of Dussumier. This could mean that the label was written at the same time (late 1860's, early 1870's) as the register. Apart from the original text on the label there is some more information (probably from a later date) written on the label in a coarse hand in bold pencil (not in Indian ink as supposed by Frazier & Matyot, 2010, p. 38): near the top middle and right hand: 'nigrita D & B.' and in the lower left corner, a bit above the bottom, in about the place where RMNH labels generally show the collector, 'Dussumier' (Grünwald, 2009). Thus, there is a good chance that the old label on the bottle of RMNH 3231 is not the 'original' label as stated by Hoogmoed et al. (2010), and that it possibly stems from after 1835 as suggested by Frazier & Matyot (2010). Anyway, it is a nearly true copy of the data in the first RMNH register and of the data provided by Temminck & Schlegel (1834). The only questionable matter that remains is why the name *Test. dussumieri*, mentioned by Temminck & Schlegel (1834) and Gray (1831 b) does not appear in the register or on the label. Thus, most likely the old label that is nowadays on the outside of the bottle of RMNH 3231 was not seen by Gray, so he must have based himself on other information.

Conclusion

Based on the arguments presented above I come to the conclusion that none of the allegations presented by Frazier & Matyot (2010) about the identity and the validity of the locality from which RMNH 3231 came can be substantiated by any hard evidence and therefore should be regarded as void. These allegations should not be taken into account in the discussion on Case 3463, trying to get accepted the unnecessary designation of a neotype for *Testudo gigantea* Schweigger, 1812.

Additional references

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No further comments on Case 3463 will be accepted for publication after 1 May 2011 unless they contain substantial new evidence that is likely to affect the vote.

Comment on the proposed conservation of usage of genus *Rhynchotherium* Falconer, 1868 (Mammalia; Proboscidea) by designation of *Rhynchotherium falconeri* Osborn, 1923 as the type species

(Case 3515; see BZN 67: 158–162, 256–257)

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After a review of the opinion raised by Lucas (BZN 67: 158) and the comments by Morgan (BZN 67: 256), we strongly support the proposal for the conservation of the name *Rhynchotherium*, since the morphological characters are distinctive in the New World gomphotheres of the Pliocene epoch, e.g. a relatively short mandible, broad and with a symphysis obliquely depressed downwards and two lower tusks laterally compressed or deeply oval, often bearing external enamel bands.

The assignation of a holotype was confused from the original designation by Falconer (1868), since he used the cast of *R. tlascalae* for naming a new taxon [Tobien (1973, p. 237) indicated that this cast pertained to an individual from the genus *Gomphotherium*, not to *Rhynchotherium*]. This error was seen by Osborn (1936) and he tried to correct it by suggesting *R. browni* as a neotype (see Osborn, 1936 fig. 452), however this proposal was discarded and all the specimens retained in the genus *Rhynchotherium*, since the most important diagnostic characters were those mentioned above for the mandible and the tusks, which enabled a reliable identification as it could be seen in recent discoveries of this taxon; e.g. Mexico (Alberdi & Corona, 2005; Corona & Alberdi, 2006).

We have authored several taxonomic papers in which we discussed the specific and generic statuses within GOMPHOTHERIIDAE and its taxonomy (Alberdi et al., 2002, 2004, 2007b, 2008, 2009; Corona & Alberdi, 2006; Prado et al., 2002, 2005). We have also authored two papers about phylogeny and biogeography of trilophodont gomphotheres in which the genus *Rhynchotherium* is included (Alberdi et al., 2007a; Prado & Alberdi, 2008). In those two last papers we rejected the hypothesis that *Rhynchotherium* was a direct ancestor of South American gomphotheres, and supported the hypothesis that *Sinomastodon* is their sister group.

For the proposal to conserve the genus name and for the taxonomic stability of the group, a new type species is warranted, and certainly we do agree that in the first instance it could be *R. falconeri*. However, this would not rule out further studies of the group in order to determine a type species that better represents the main features of this taxon.

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Comment on the proposed conservation of usage of *Mastodon waringi* Holland, 1920 (currently *Haplomastodon waringi*; Mammalia, Proboscidea) by designation of a neotype

(Case 3480; see BZN 66: 164–167, 358–359; 67: 96, 181–182, 333)

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We certainly recognise the problem suggested by Lucas (BZN 67: 181) in regard to the name differences between the South American gomphotheres. However, before dealing with the question as to whether or not a neotype for *Mastodon waringi* should be proposed, we consider that a decision in regard to the validity or otherwise of the genus name *Haplomastodon* should be made. In that regard, the following statements should be considered during the discussion of Lucas's proposal:

(1) Supporting the use of the name *Haplomastodon* Hoffstetter, 1950, characterised by the absence of foramina transversa in the atlas. This name was proposed as a subgenus of *Stegomastodon* with type species *Mastodon chimborazi* Proaño, 1922.

The diagnostic characters of the subgenus *Haplomastodon* indicated by Hoffstetter (1950, 1952) are not significant, because they are the same as those of the genus *Stegomastodon*, and some of them (i.e. open foramina) are quite variable, as pointed out by Simpson & Paula Couto (1957). These authors also looked in detail for the differences between *Stegosmastodon* and *Haplomastodon*, finding that there were really very few and concluding that the latter genus would be a morphological intermediate between *Cuvieronius* and *Stegomastodon*; the same conclusion was stated by the authors in their Summary (p. 185), i.e. *Haplomastodon* is believed to be about as closely related to *Cuvieronius* as to *Stegomastodon*.

Prado et al. (2005), in agreement with Simpson & Paula Couto (1957), considered that the genus *Haplomastodon* could not be clearly differentiated from *Stegomastodon*. The character of the foramina transversa in the atlas and axis vertebrae is variable in the specimens from the Araxá locality, that is, it could be either present or absent (Simpson & Paula Couto, 1957:167–168). The distinguishing characters between these genera vary greatly in respect to the animal's age and are, therefore, not very good; both genera are very similar in the skull shape – elephantoid type, adult tusks usually straight or slightly curved at the tip and the mesial part of the maxilla with hemimaxilla straight and in contact (not divergent as in *Cuvieronius*).

Alberdi & Prado in their studies of gomphotheres from several localities of South America (Alberdi et al., 2002, 2004, 2007, 2008, 2009; Alberdi & Corona, 2005; Prado et al., 2002, 2005; Prado & Alberdi, 2008) found specimens either with or without foramina transversa in the atlas within the same species. They also observed that the only differences between the two genera (*Haplomastodon* and *Stegomastodon*) are found in the morphology of premolar and molar occlusal surfaces, where patterns (trefoils) are more complicated, or there are more accentuated plications (pticostilia) in *Stegomastodon* than in *Haplomastodon*, and also there are certain angulations on the loph(id)s more accentuated in *Stegomastodon* than in *Haplomastodon*, the last with more single trefoils (posttrites and prettrites) less developed than in *Stegomastodon* where they are more complicated. The differential characters of both genera are not enough to separate those taxa at the genus level, but only as subgenera.

Consequently, Prado et al. (2005) synonymised *Haplomastodon* with *Stegomastodon*, including two species: *S. waringi* and *S. platensis*.

(2) The proposal of *Mastodon waringi* as the neotype for the genus *Haplomastodon* by Lucas (BZN 66: 164) and supported by Pasenko (BZN 67: 96) should be overruled since the main problem would be confirming whether or not *Haplomastodon* is a junior synonym of *Stegomastodon* as we assessed. Furthermore, Ferretti's (BZN 66: 358) proposal for designating *Mastodon chimborazi* as a neotype should be also questioned on the same grounds as those mentioned above, unless a decision is taken with regard to changing the generic status of *Haplomastodon*.

Additional references

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